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J. R. HIND, Esq., F.R.S., President, in the Chair.

Squire Thornton Stratford Lecky, Esq., 1, Morningside Road, Bootle, Liverpool;
James Henry Mitchiner, Esq., Burgos House, Sydenham Road North, Croydon; and
The Rev. T. R. Terry, M.A., Magdalen College, Oxford;

were balloted for and duly elected Fellows of the Society.

On the Determination of the Value of the Parallactic Inequality in the Motion of the Moon. By James Campbell, Esq., and Edmund Neison, Esq.

In the *Monthly Notices* for November 1880, there is a paper by Mr. Stone, F.R.S., criticising a paper of ours which appeared in the *Monthly Notices* for May and June 1880, and is entitled, "On the Determination of the Solar Parallax by means of the Parallactic Inequality in the Motion of the Moon." This paper by Mr. Stone calls for a reply from us.

Mr. Stone's paper may be divided into three portions, under the following heads—

(1) An account of the method adopted by him in his determination of the value of the parallactic inequality published in 1867.
(2) A demurrer to the manner in which the effect of the variation in the irradiation at the lunar limb is deduced in our investigation.
(3) A criticism of the hypothesis of the possible existence of an equality of long period in the apparent value of the coefficient of the parallactic inequality.

These three portions we propose to take in order.

(1) Mr. Stone states that we have formed an incorrect idea of the method employed by him in the investigation of which the only published account is the short abstract in the *Monthly Notices* for May 1867, an account which, though professing to describe the method which was followed, yet makes no reference to the additional precautions and corrections which it seems were actually employed by Mr. Stone. Through interpreting in the most natural manner the account given in this abstract, it seems that its imperfect nature has led us to form an incorrect idea of the method employed. This we must regret. Yet we submit that the carefully guarded manner in which we describe, on pp. 407–409 of our paper, the conclusions which we draw as to the probable method followed by Mr. Stone, and the clear manner in which we restrict our criticisms to Mr. Stone's value as thus hypothetically interpreted, must be regarded as effectually vindicating us from the unnecessary strictures in this paper by Mr. Stone.

We accept without demur Mr. Stone's statement that the specific corrections hypothetically applied by us to his result are really inapplicable. This naturally follows from the fact that Mr. Stone's investigation had a different basis to what we had been led to imagine. We learn now from Mr. Stone what is the real basis of his investigation, and consequently can form, now, a clear idea of the value of the result. The only conclusion which it seems can be drawn is that the value obtained by Mr. Stone must require the application of a considerable number of large corrections, and even then can only be regarded as of most imperfect nature.

Thus, from Mr. Stone's account, it seems, that actually passing over the great mass of indirect comparisons between observation and Hansen's tables, he employed the far inferior series of comparisons between observation and Burckhardt's tables, merely applying some small corrections to render these latter tables somewhat less faulty. Further, it seems that in round numbers more than three-fourths of the observations employed by Mr. Stone were compared with Burckhardt's tables, and only one-fourth with Hansen's tables. Moreover, it would appear that some two-thirds of the observations made use of by Mr. Stone were made with the Greenwich altazimuth instrument, and only about one-third with the transit circle.

Now, the errors in Burckhardt's tables are so large and so numerous that, to render the tables fit for use in deducing corrections to any of the lunar inequalities, it is absolutely essential that a considerable number of corrections should be applied. Moreover, in most cases these corrections must be applied to the individual observations, and cannot be adequately applied to the mean results. This is a point which we strongly urge, and is founded on the considerable experience which one of us has had in the comparison of these tables with observations. The errors

introduced by the employment of these tables are rectified in a very small degree by merely applying a correction to the mean results for "errors in the coefficient of Burckhardt's variation, annual equation, and term 3D." (Mr. Stone does not tell us what were the separate corrections he deemed these coefficients to require.)

Besides these errors in the values of the coefficients of the terms actually employed by Burckhardt in his Lunar Tables, these tables are vitiated by the imperfections due to the omission of numerous terms found by subsequent theorists to have sensible coefficients. These omissions will introduce systematic errors into any results based on his tables. It may be urged that in the mean of nine or thirteen years' observation these errors may mutually destroy each other. They *may*: but this is a point which must be proved, not assumed.

But the greatest objection to the results obtained by Mr. Stone lies in the fact that they practically are founded on the altazimuth observations, and mainly on the altazimuth observations compared with Burckhardt's tables.

The altazimuth observations are liable to so many systematic errors that they are entirely unfitted for use in determining the value of the parallactic inequality, especially when they are compared with faulty tables. The observations for the positive and negative values of the parallactic inequality are systematically made with this instrument under entirely different conditions, and are affected, therefore, by every systematic error inherent to such a condition. The observations at the two quarters are not only made at different times of the night, one long before and the other usually long after midnight, but they are made on opposite sides of the meridian, and in entirely different portions of the heavens. All these must tend to introduce grave systematic errors. Further, from the conditions under which the observations are made, not only will there be grave systematic errors introduced through the variations in the semi-diameter of the Moon, but there will be also serious systematic errors introduced through the corrections both for parallax and refraction. The difficulties in dealing with the variations in the apparent diameter of the Moon seem enormous. The diameter of the Moon derived from observations in altitude is known to be entirely discordant with that derived from observations in azimuth, differing, in fact, by over one second; and this, whilst conclusively establishing the existence of serious systematic errors in the observations, renders it quite inadmissible to apply a constant correction for semi-diameter, as is done by Mr. Stone. Which is the true correction to be employed, that furnished by the observations in altitude, or that furnished by the observations in azimuth? The resulting values of the parallactic inequality will differ by over half a second. And if neither, but some intermediate value, how can it be determined what intermediate value is correct? Moreover, though the personalities in observing diameters with the transit circle have

been shown to be small, those in observations with the altazimuth are known to be very large.

For this reason we consider that the altazimuth observations are unfitted for use for the purpose of determining the value of the parallactic inequality in the motion of the Moon, unless care be taken to eliminate all these sources of systematic error by the application of adequate corrections to the separate observations; and we believe that in this opinion we shall be supported by all those astronomers who have had experience in the comparison of these altazimuth observations with an adequate lunar theory.

As, from Mr. Stone's account of his investigation, he only applied to mean results assumed corrections for errors in the variation, annual equation, and term 3θ (Mr. Stone's 3D), together with a constant correction for semi-diameter, it would appear that the corrections he has applied are altogether inadequate to properly eliminate the various sources of systematic error affecting the altazimuth observations.

(2) The second point raised by Mr. Stone is a very simple one. In our paper we determined the coefficient of the irradiation constant from the equations—

$\delta P + 0{\cdot}75\ \delta I$ = apparent value of parallactic inequality from summer observations.

$\delta P + 0{\cdot}25\ \delta I$ = apparent value of parallactic inequality from winter observations.

Mr. Stone objects that the values taken for the coefficients of δI are assumptions, and that their correctness must be proved. He shows that by varying the ratio between these two coefficients you make important changes in the value of the true parallactic inequality. This objection would be perfectly valid were the ratio between these two coefficients a mere assumption, as imagined by Mr. Stone. But this is not so. On p. 445 of our paper it is clearly stated that these coefficients, and therefore the ratio between them, were derived from the observations themselves. This ratio of three to one arises practically from the fact that three times as many observations are made on a bright sky in the summer as are made in the winter. The actual figures given by the observations range usually within a small distance on either side of the mean values 0·25 and 0·75.

It has been already stated that the coefficient i of the constant δI was assumed to vary directly as the brightness of the sky at the epoch of the transit of the Moon. This leads to results the same as employed by Prof. Newcomb. But even if a different law be assumed, it will be found that the ratio between the two coefficients is not materially affected. Originally, a somewhat different law was assumed by one of us in these investigations, and this result was found to hold; but it was abandoned because, on comparison with the results furnished by the observa-

tions, it was found that the variation in the irradiation increased at a greater rate than had been assumed.

There is, however, some additional information on this point which may now be given; which, indeed, would have been included in our former paper had it not been deemed inadvisable to increase its already great length. This is a proof of the accuracy of the law we have assumed for the variation in the irradiation at the limb; the law being that the coefficient i varies directly as the brightness of the sky, or that the irradiation at the limb varies directly as the contrast in brightness between the limb of the Moon and the sky it is seen against, or inversely as the brightness of the sky.

In the reduction of the Greenwich lunar observations which has been undertaken by one of us, every observation is supposed to require the small correction $i\delta\text{I}$, where i varies from 0 to 1·5 according to the law stated. Let the observations be divided into four groups; those for which the value of i lies between the limits—

$i = 1·5$ and	$i = 1·2$	mean $i = 1·35$
$i = 1·2$	$i = 0·9$	$i = 1·05$
$i = 0·9$	$i = 0·3$	$i = 0·60$
$i = 0·3$	$i = 0·0$	$i = 0·15$

From each of these groups let δI be determined, and if each group yields the same value, then the assumed law must accurately represent the true law; but if they be discordant, then the coefficient i must be made to vary according to some different law which will make all these values alike. It is not difficult to eliminate from the observations for any group of years nearly all the other corrections, leaving merely δI. The Greenwich observations for the nine years 1868–1876 were thus reduced, leaving out the last group, as the coefficient of i is too small to give trustworthy results. The number of observations included in the first group were but small, so that its weight was small. Of course, the real importance lies in the results obtained from the two middle groups, for if these give identical values of δI, then the assumed law is proved. The results are—

For mean value of $i = 1·35$	$\delta\text{I} = -1''·30$	Weight 1
$= 1·05$	$= -1·25$	5
$= 0·65$	$= -1·32$	7

This completely confirms the sufficiency of the assumed law. It may be added that these results are based on the complete series of meridian results for the period specified, or on nearly one thousand observations.

Mr. Stone has also criticised the value we find for δI. He states that even after applying the constant corrections given by us, the separate values are very discordant. Apparently

Mr. Stone has overlooked the following paragraph in our paper (pp. 464, 465)—

"When the values assigned to the corrections δV, δM, δK, δl, come to be substituted in the expressions for δI during the different years, it will be found that there still remain considerable outstanding errors. These are partly due to the fact that constant values have been assigned to these corrections, whereas the observations show that they are subject to some variation, due to the fact that Hansen's tables require one or two more corrections not included in the preceding expression. Strictly speaking, in each year should have been used the values found for δV, δM, δK, δl, from the observations of that year, as in that manner these neglected corrections would have been practically eliminated. The method actually adopted for determining the value of P gives results which are sensibly as accurate as if the stricter method had been employed, for the corrections are multiplied by such small coefficients that slight variations may be neglected. When the expressions for δI are considered, the use of the stricter method markedly reduces the values of the outstanding errors in the years 1876, 1869, 1868." [Almost the only large outstanding errors.] "But it must be remembered that the apparent errors of these years are much exaggerated by their being derived from equations in which δI is multiplied by a small coefficient, so that in dividing by this coefficient the apparent errors are increased to double, triple, or even quadruple their true amount. In the value for the irradiation which has been derived from the observations and actually employed, all the equations are grouped together, and appear with their proper weight according to the largeness of their coefficients of δI. The result, therefore, is entitled to far more weight than may appear from a consideration of the outstanding discordances, so many of which have been thus exaggerated."

It may be added that the results were computed on both systems, and found to agree. Since then the principal cause of the variation in the apparent value of δM and δK (for the latter, Prof. Newcomb's value was used in our paper) has been found to arise from the need of a correction to the term with the argument α or the mean anomaly of the Moon. This was surmised in our paper.

Therefore, as it has been shown that the law of the variation in the irradiation at the limb, the actual amount of this variation, and the ratio between the coefficients with which it appears in the final equations are all derived directly from the observations themselves, it must be held that the correction for the effect of this variation in irradiation is also furnished directly by the observations, and is altogether free from that arbitrary character imagined by Mr. Stone.

We hold, therefore, that we have vindicated this portion of our investigation from the demurrer raised against it by Mr. Stone.

(3) In his criticisms on the portion of our paper dealing with the hypothetical forty-five-year inequality in the apparent value of the parallactic inequality, Mr. Stone seems to have entirely misunderstood our position, and it might be imagined that we supposed our paper to contain a proof of the existence of this new term. But that is a complete misconception, as throughout we refer to this inequality as purely hypothetical, and one whose existence has yet to be proved. (See for instance page 467 lines 16–28, and page 468 lines 6–8.) As we so clearly state in our paper, we do not regard the existence of this term as established, for reasons we have already given, but we do consider that there is strong evidence in favour of the possible existence of such an inequality.

Let us recapitulate this evidence.

Sir G. B. Airy had reduced on a uniform system, and by reference to uniform lunar tables, the entire series of Greenwich lunar observations made between 1750 and 1851; and from these observations there have been deduced twenty-one values of the coefficient of the apparent parallactic inequality, these values being strictly comparable *inter se*, and each being based on some six or seven hundred observations, spread over a period of nine years. When these values come to be compared, it is found that they exhibit grave discordances amounting to over a second of arc in many cases, but that these discordances are obviously periodical in form. If these discordances be assumed to be due to the existence of a hitherto unsuspected term in the motion of the Moon, giving rise to an apparent inequality of forty-five years' period in the values found for the parallactic inequality, all these discordances disappear from twenty of these nine-year groups, and the only discordance is for the small group of years 1847–1851. There can be no doubt about this constituting strong evidence in favour of the existence of such an inequality. It is not the case, as imagined by Mr. Stone, that the only evidence in its favour lies in the early observations made prior to 1816; for a reference to the table on page 403 of our paper shows serious discordances in the observations between 1816 and 1846 which are completely removed when the inequality is introduced. These as strongly support its existence as any of the earlier groups. We have, therefore, the observation for the ninety-seven years 1750–1846 in its favour, and only those of the five years 1847–1851 against it. Mr. Stone urges that the observations of the period 1847–1851 are superior to those of the preceding century. Undoubtedly, but they are not sensibly superior to those made between 1835–1846, or much better than those between 1816–1835.

Further, it is found that when the observations made between 1862–1876 are reduced and compared with Hansen's tables as corrected, they give a value of the apparent parallactic inequality which is in perfect accord with the assumed existence of this forty-five-year inequality. It is here, however, that the weak

point appears, and is duly noted by us; these observations are not only made with a different instrument, but they are not referred to the same tables as the observations employed by Sir G. B. Airy, and the two results are not *strictly comparable.* It is true that the fundamental differences between the two tables (Airy and Hansen *both corrected from observation*) are not so large as to render them likely to be of much importance, and the two instruments were of analogous nature, but these differences do decrease the weight to be attached to the close accord found to exist between the results.

Mr. Stone, however, to show that this hypothetical inequality has no real existence, brings forward the values which he has deduced for the apparent value of the parallactic inequality, by comparing the altazimuth observations of the Moon for 1848–1861 with Burckhardt's lunar tables, and for the years 1862–1878 with Hansen's tables. But as these observations are not only made with an entirely different description of instrument, they are neither comparable with the results obtained by Sir G. Airy or by ourselves from the meridian observations, nor, as they are referred to entirely different tables, are they even comparable with each other. For, be it remembered, these values which are being discussed are not the values of the coefficient of the true parallactic inequality, but of the apparent inequality which is affected by the variations in the irradiation at the limb, and may be therefore entirely different in different instruments. If the results quoted by Mr. Stone were comparable, then they would be utterly discordant with the hypothesis of a forty-five-year inequality. But they most certainly are not comparable. The results deduced through the exceedingly faulty tables of Burckhardt, and affected by all the consequent systematic errors, cannot be compared with the results deduced through the more exact tables of Hansen without the application of a far more extensive series of corrections than has been applied by Mr. Stone;* and the results deduced from the altazimuth observa-

* With the view of showing the justness of this statement that the corrections applied by Mr. Stone to the results from Burckhardt's tables are entirely insufficient to eliminate the errors introduced by the imperfection in the theoretical basis of the tables, we have computed the true corrections to be applied to the results for the three years 1854–56, taking into account the errors in some twenty-two terms, instead of only three. The results are:—

	Stone.	Neison and Campbell.		
	Correction for error in 3 terms.		Correction for remaining 18 terms.	
1854	= + 1″·56	+ 1″·47	= − 1″·85	Sum = − 0″·38
1855	= + 1·56	+ 1·62	= − 1·96	= − 0·34
1856	= + 1·56	+ ·98	= + 0·97	= + 1·95

In these particular years the errors are not near these maximum values, and it may be stated that the constant correction used by Mr. Stone of + 1″·56 is a variable quantity which may take all values from about + 3″·5 and − 1″·5.

tion cannot be compared with the results of the meridian observations, as they are affected by constant errors due to the variation in the irradiation at the limb, which must differ in the two different classes of instruments, even had not the results derived from the altazimuth observations been affected by serious systematic errors.

Mr. Stone, however, has appealed to the observations of the period 1850–1860 as being decisive against this forty-five-year inequality. Are they really so? This is a matter admitting of proof, for there exists a considerable number of observations made with the transit circle and indirectly compared with Hansen's tables. They form, in fact, the very data which we had imagined Mr. Stone to have employed in his investigation, being so much superior to those actually employed by him. These observations are given in the volume of Greenwich Observations for 1859, and from them we select all the observations made with the transit circle, and comprised within the years 1851–1858. They furnish us with results strictly comparable with those for the years 1862–1876 made use of by us in our previous paper.

The results are as follows:

1851	43 observations	$\delta(P) = -2''.03$
1852	31	$= -3.17$
1853	42	$= -1.50$
1854	45	$= -2.45$
1855	36	$= -1.80$
1856	37	$= -2.49$
1857	38	$= -2.46$
1858	53	$= -2.65$
	325 observations	$\delta(P) = -2.32$

To reduce this result to what would have been obtained had they been compared directly with Hansen's tables, it is necessary to apply the constant correction for the difference between the two tables at midnight and at the epoch of observation. This amounts to (see page 401 of our paper)—

$$\tfrac{1}{9}\,\Delta V + \tfrac{1}{25}\Delta Q = -0''.52.$$

The result is then the value which would have been obtained had the observations been compared directly with Hansen's tables. It is

$$(P) = -\{126''.46 - 2''.32 - 0''.52\} = -123''.62.$$

This value has now to be corrected for the errors in Hansen's

tables. These have been approximately calculated, and the mean correction for the eight years is

$$+0{\cdot}10\ \delta V - 0{\cdot}13\ \delta M + 0{\cdot}07\ \delta A = +0''{\cdot}14.$$

The correction for the term of the form $\delta K \sin k$, which gives the correction required by Hansen's tables for errors in the equation of the centre and the Jovian evection, was calculated, but in the mean of the eight years nearly vanishes; it is—

$$+\kappa\ \delta K \quad = -0''{\cdot}03.$$

For the correction required by the semi-diameter we have taken that given in the Greenwich Observations for 1863, namely

$$+s\ \delta S \quad = -0''{\cdot}18.$$

This is somewhat greater than the value found by one of us for the period 1851–1858. The sum of these corrections amounts to

$$+0''{\cdot}14 - 0''{\cdot}03 - 0''{\cdot}18 \qquad = -0''{\cdot}07.$$

Therefore, the value of the apparent parallactic inequality will be

$$(P) = -\{123''{\cdot}62 - 0''{\cdot}07\} = -123''{\cdot}55.$$

This is the result furnished by the 325 meridian observations made during the eight years 1851–1858.

It is easy now to compare this result with those obtained in our previous paper. We have added the correction required for the forty-five-year inequality—

1851–1858	$(P) = -123''{\cdot}55 - \frac{4}{7}B$
1862–1869	$= -125{\cdot}09 + \frac{4}{5}B$
1870–1876	$= -125{\cdot}13 + \frac{4}{5}B.$

Giving to B its value, $B = +1''{\cdot}20$, these become

1851–1858	$(P) = -124''{\cdot}23$
1862–1869	$= -124{\cdot}09$
1870–1876	$= -124{\cdot}13.$

To these may be added Airy's value

1751–1846	$(P) = -124''{\cdot}13.$

It will be evident, therefore, that the entire series of meridian

observations unite in supporting this hypothesis of a forty-five-year inequality. More than that we do not wish to say at present, unless it is to add that if this inequality really exists it must be possible to find out its origin and calculate from theory its value.

In reducing these observations, care was taken to determine the value they furnish for the irradiation constant δI so as to enable the value of the true coefficient of this parallactic inequality to be obtained. They gave the value

$$0{\cdot}50\,\delta I = -1''{\cdot}73 - \kappa\delta K - t\delta T.$$

The value of the correction depending on the term $\delta K \sin k$ was calculated, and the mean for the eight years was

$$\kappa\delta K = +0''{\cdot}46.$$

The term depending on the factor $t\,\delta T$ or on the systematic errors in the Greenwich clock star places, was calculated to be approximately

$$t\delta T = +0''{\cdot}72.$$

Hence

$$0{\cdot}50\,\delta I = -1''{\cdot}73 + 1''{\cdot}18 = -0''{\cdot}55.$$

Therefore

1851–1858	$\delta I = -1''{\cdot}10$
1862–1869	$= -1{\cdot}06$
1870–1876	$= -1{\cdot}18.$

The weight of the first of these is considerably smaller than that of the last two groups.

Employing these values of the irradiation correction, we have

1851–1858	$\delta P = (P) + 0{\cdot}44\,\delta I$	$= -124''{\cdot}72$
1862–1869	$= (P) + 0{\cdot}46$	$= -124{\cdot}62$
1870–1876	$= (P) + 0{\cdot}50$	$= -124{\cdot}66.$

The accordance between the different results is complete, and leaves it necessary for us to add nothing.

(4) In concluding this paper it may be as well perhaps to state that further information on the true value of the parallactic inequality and on the rate and amount of the variation in the irradiation at the limb, but based on the entire available Greenwich Observations, will be furnished in all detail in a memoir on the corrections required by Hansen's lunar tables which is now